Name: ________________________________

ECON3500 Econometrics and Applications

Exam 3 Practice Exam (CH8–10, 12) — SOLUTIONS

There are 50 total points. This practice exam is longer than the actual exam (which is 75 minutes). Show your work to receive full credit. **Regression output for Question 5 appears on the last page.**

1. *(12 points)* A researcher investigating the effect of healthcare spending on health outcomes in the United States has data for 50 states over 20 years (1995-2014). She estimates the following regression using OLS with fixed effects:

$$ln(health)_{it} = \beta_1 ln(spending)_{it} + \beta_2 unemployment_{it} + \alpha_i + \phi_t + u_{it}$$
$$i = 1, ..., 50; \quad t = 1, ..., 20$$

where *health* is a health outcome measure (e.g., life expectancy), *spending* is per capita healthcare expenditure, *unemployment* is the state unemployment rate, α_i represents state fixed effects, and ϕ_t represents year fixed effects.

(a) What is the purpose of including state fixed effects (α_i) and year fixed effects (ϕ_t)? What unobserved factors might each type of fixed effect capture? *[4 points]*

State fixed effects (α_i) control for all **time-invariant** characteristics of each state—e.g., geography, long-standing health infrastructure, cultural attitudes toward healthcare, baseline demographics, climate.
Year fixed effects (ϕ_t) control for **nationwide shocks and trends** that affect all states equally in a given year—e.g., federal healthcare policy changes (like the ACA), macroeconomic cycles, advances in medical technology, nationwide health campaigns.

(b) The results below show the within-estimates (with fixed effects). Discuss the meaning of the coefficient on healthcare spending and its statistical significance. What is the effect of a 10% increase in healthcare spending on health outcomes?

```
ln(health) = 0.284 * ln(spending) + 0.042 * unemployment
             (0.067)                (0.018)

R^2 = 0.876
N = 1000 (50 states × 20 years)
```

[5 points]

This is a **log-log** model (both *health* and *spending* are in natural logs).
Interpretation: A 1% increase in per capita healthcare spending is associated with approximately a 0.284% increase in health outcomes, holding unemployment constant and controlling for state and year fixed effects.
Statistical significance: $t = 0.284/0.067 = 4.24$. Since $|4.24| > 1.96$, the coefficient is statistically significant at the 5% level.
Effect of a 10% increase: $0.284 \times 10 = 2.84\%$ increase in health outcomes.

(c) Suppose the regression is re-estimated **without** state and year fixed effects (simple pooled OLS). The coefficient on $ln(spending)$ becomes 0.512 with SE = 0.043. Why might the coefficient differ so much when fixed effects are removed? What does this suggest about the relationship between healthcare spending and unobserved state characteristics? *[3 points]*

Without fixed effects, the coefficient jumps from 0.284 to 0.512, suggesting **positive omitted variable bias** in pooled OLS. States with higher healthcare spending likely also have unobserved characteristics that independently improve health outcomes (e.g., wealthier states can afford more healthcare AND have better living conditions; states with better governance invest in healthcare AND provide other public goods). The fixed effects absorb this confounding, producing a smaller and presumably less biased estimate.

2. *(11 points)* A researcher studies the effect of class size on student achievement using data from schools in developing countries. She hypothesizes that smaller classes improve test scores, but student selection is a concern—schools with better teachers may both have smaller classes and higher achieving students.

$$test\_score_i = \beta_0 + \beta_1 class\_size_i + \beta_2 teacher\_experience_i + u_i$$

She proposes using **random assignment of students to classroom** as an instrumental variable for class size. This ensures class size variation is exogenous.

(a) For this instrument to be valid, what assumptions must hold? For each assumption, indicate whether you can test it with data. If you can, explain how; if you cannot, explain why not. *[4 points]*

Relevance: Random assignment must be correlated with actual class size. **Testable**—run the first stage regression and check the F-statistic ($F > 10$ rule of thumb).
Exogeneity (independence): Random assignment must be uncorrelated with the error term (other determinants of test scores). **Not directly testable**, but arguable: if assignment is truly random, it should be independent of student ability, family background, etc. Can partially assess by checking balance on observables.
Exclusion restriction: Random assignment must affect test scores *only* through class size. **Not testable**—must argue on institutional grounds.

(b) OLS results show: coefficient on class size = -0.15 (SE = 0.08). IV results show: coefficient on class size = -0.42 (SE = 0.18). Compare these estimates. Why might IV yield a stronger effect? *[4 points]*

OLS: $\hat{\beta}_1 = -0.15$, $t = -0.15/0.08 = -1.875$. Since $|1.875| < 1.96$, **not significant** at 5%.
IV: $\hat{\beta}_1 = -0.42$, $t = -0.42/0.18 = -2.33$. Since $|2.33| > 1.96$, **significant** at 5%.
IV is larger in magnitude. OLS is likely biased toward zero because schools with better resources may have both smaller classes AND higher-achieving students (positive OVB), masking the true negative causal effect. Measurement error in class size could also attenuate OLS toward zero.

(c) Provide one specific threat to the validity of using random assignment as an instrument for class size. *[3 points]*

Any one of the following is sufficient:
- **Peer composition:** Random assignment changes *who* your classmates are, not just how many. Peer effects could affect test scores independently of class size, violating the exclusion restriction.
- **Teacher assignment:** Schools may assign different teachers to different-sized classes, and teacher quality affects scores independently.
- **Non-compliance:** Students may switch classrooms after assignment, weakening the first stage.

3. *(13 points)* A policymaker wants to evaluate the effect of a new job training program on employment rates. Some states adopted the program in 2015, while others did not. The researcher has employment data for 20 states over 2010-2018 (before and after 2015).

(a) Explain why a simple before-after comparison (comparing 2015-2018 to 2010-2014) would be problematic for estimating the causal effect of the training program. *[3 points]*

A simple before-after comparison confounds the program effect with other changes occurring over the same period. Many factors could have changed between 2010–2014 and 2015–2018: macroeconomic conditions, federal employment policies, demographic shifts, industry changes. Without a control group, we cannot attribute changes in employment solely to the training program.

(b) Propose a difference-in-differences (DiD) model to estimate the causal effect. Define any new variables. Identify which coefficient represents the program effect. *[5 points]*

$$Employment_{it} = \beta_0 + \beta_1\, Treat_i + \beta_2\, Post_t + \beta_3\,(Treat_i \times Post_t) + u_{it}$$

Where: $Treat_i = 1$ if state adopted the program, 0 otherwise; $Post_t = 1$ if year $\geq$ 2015, 0 otherwise.

β_3 **is the DiD estimate**—the causal effect of the job training program on employment rates. It captures the *additional* change for treatment states beyond what the control states experienced.

β_0: avg. employment in control, pre-period. β_1: pre-existing group difference. β_2: time trend for control group.

(c) What key assumption must hold for your DiD model to measure a causal effect? Explain in plain English. *[2 points]*

Parallel trends: In the absence of the job training program, employment rates in adopting and non-adopting states would have followed the same trend over time. In plain English: if the program had never been implemented, the gap in employment between the two groups of states would have stayed the same.

(d) What concerns do you have about the external validity of these results? Under what conditions would these results generalize to other states or contexts? *[3 points]*

- **Selection into treatment:** States that adopted may differ systematically (more resources, different labor markets, greater political will). Results may not generalize to non-adopting states.
- **Economic conditions:** Results depend on 2010–2018 conditions; the program could perform differently in other macroeconomic environments.
- **Implementation & population:** How the program was run, and the demographics/industry mix of adopting states, may not be representative.

4. *(5 points)* Consider the following causal diagram for the relationship between education and earnings:

Ability → Education → Earnings
Ability → Earnings
Education → Job Type → Earnings

In this DAG, Education causes Earnings both directly and through Job Type. Ability is an unobserved variable that causes both Education and Earnings.

(a) List all paths from Education to Earnings. For each path, state whether it is a **causal (front door)** path or a **backdoor** path. *[2 points]*

1. Education → Earnings — **causal (front door)**, direct effect
2. Education → Job Type → Earnings — **causal (front door)**, indirect effect
3. Education ← Ability → Earnings — **backdoor path** (non-causal, creates confounding)

(b) What variable(s) should you control for to identify the causal effect of Education on Earnings? What variable(s) should you **not** control for? Explain why for each. *[2 points]*

Control for: Ability — closes the backdoor path (Education ← Ability → Earnings), removing confounding/OVB.
Do NOT control for: Job Type — it is a **mediator** on the causal path Education → Job Type → Earnings. Controlling for it blocks part of the total causal effect.

(c) Suppose Ability is unobserved and cannot be measured. Name one identification strategy from this course that could help estimate the causal effect of Education on Earnings despite this unobserved confounder. Briefly explain how it would work. *[1 point]*

Instrumental Variables (IV/2SLS): Find an instrument correlated with Education but not directly with Earnings (e.g., distance to college, compulsory schooling laws). First stage regresses Education on the instrument; second stage uses predicted Education to estimate the causal effect on Earnings.

5. *(9 points)* An education researcher studies factors affecting standardized test scores using data from 620 elementary school students. She is interested in whether the effect of small class sizes differs for boys and girls. **Regression output appears on the last page of this exam.**

Variable	Description
score	Standardized test score (points)
ln_income	Natural log of family income (dollars)
small_class	$= 1$ if class size ≤ 18 students
female	$= 1$ if female
small_female	$small\_class \times female$ (interaction term)

$$score_i = \beta_0 + \beta_1\, ln\_income_i + \beta_2\, small\_class_i + \beta_3\, female_i + \beta_4\, small\_female_i + u_i$$

(a) What type of model is this with respect to *score* and *income*? Circle one. Interpret the coefficient on *ln_income*. If family income increases by 10%, what is the predicted effect on test scores? *[3 points]*

Type (circle one): level–level log–level level–log log–log

This is a **level-log** model (*score* in levels, *income* enters as ln).
$\hat{\beta}_1 = 12.840$: A 1% increase in family income is associated with a $12.840/100 = 0.128$ point increase in test scores, holding class size, gender, and their interaction constant.
10% increase in income: $10 \times 0.128 = \mathbf{1.28}$ point increase in test scores.
Note: The exact calculation gives $12.840 \times \ln(1.10) = 12.840 \times 0.0953 = 1.22$ points. Either answer is acceptable.

(b) Using the regression results, calculate the predicted effect of being in a small class for **male** students and for **female** students. Is the small class effect the same for both groups? *[3 points]*

This is a **binary** $\times$ **binary** interaction ($small\_class \times female$):
Male students ($female = 0$): effect of small class $= \beta_2 = \mathbf{4.52}$ points
Female students ($female = 1$): effect of small class $= \beta_2 + \beta_4 = 4.52 + 3.78 = \mathbf{8.30}$ points
The effect is **not the same**—it is 3.78 points larger for female students.

(c) Is the difference in the small class effect between male and female students statistically significant at the 5% level? State which coefficient you are testing and show your work. What does this tell us about whether small classes benefit boys and girls equally? *[3 points]*

Testing $H_0: \beta_4 = 0$ (coefficient on *small_female*, the interaction term):

$$t = \frac{3.780}{2.040} = 1.85$$

Since $|1.85| < 1.96$, we **fail to reject** H_0 at the 5% level. ($p = 0.065 > 0.05$.)
We cannot conclude that the small class effect is statistically different for boys and girls. However, the point estimate (3.78 points) suggests a potentially meaningful difference—the lack of significance could reflect insufficient power rather than no real difference.

QUESTION 5 — REGRESSION OUTPUT

```
. regress score ln_income small_class female small_female

      Source |       SS           df       MS      Number of obs   =       620
-------------+----------------------------------   F(4, 615)       =    107.84
       Model |  193842.60          4   48460.65    Prob > F        =    0.0000
    Residual |  276498.40        615     449.59    R-squared       =    0.4122
-------------+----------------------------------   Adj R-squared   =    0.4083
       Total |  470341.00        619     759.84    Root MSE        =    21.204

------------------------------------------------------------------------------
       score |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   ln_income |     12.840      2.150     5.97   0.000        8.618      17.062
 small_class |      4.520      1.830     2.47   0.014        0.925       8.115
      female |      1.950      1.460     1.34   0.182       -0.918       4.818
small_female |      3.780      2.040     1.85   0.065       -0.226       7.786
       _cons |    430.200     22.100    19.47   0.000      386.800     473.600
------------------------------------------------------------------------------
```